tors, appears, therefore, to the writer even better adapted to reflectors, if modified as suggested by the piercing of the plane mirror, thus permitting of the elimination of the usual small flat or mirror; and he hopes to hear of its application in the near future in the construction of the next great reflector, which is to wrest the supremacy too long held in these latter days by the giant refractors.

In conclusion, he would point out that should it be objected that the close proximity of the observers to the plane mirror might impair the definition, this inconvenience can be overcome with little loss of light by the use of a long eyepiece constructed on the plan of a compound microscope, or by the use of a Barlow lens.

1901 *June* 19.

Note in Reply to Mr. H. C. Plummer's Paper (Monthly Notices, vol. lxi. pp. 368–375). By B. Cookson.

Mr. Plummer has pointed out that in the expression deduced on p. 136 for the weight of an observation-equation of a meteor the Q term should be non-existent. As this term is generally small compared with the P term, the weights will be but little altered by its omission, and consequently the values of the unknowns in the equations of condition, which were originally derived, will be only very slightly modified. In order to see how far this is true, some of the original sets of equations have been resolved with the corrected weights, and it is found that in no case is the probable error of beginning and end of path changed by more than $0^\circ{\cdot}02$—a quite insignificant quantity. In the most unfavourable case the coordinates of the radiant are changed by just one-half of their probable error, but in other cases by less than one-fifth of their probable error. The conclusions of the paper are therefore in no way affected. The correct expression for the weight is D^2/P, as indicated by Mr. Plummer, and the quantities printed in italics in the table in my paper are superfluous.

On the Accuracy of Photographic Measures: being a Discussion of a recent Paper by M. Loewy. By H. C. Plummer, M.A.

1. In the eighth Circular of the Conférence Astrophotographique Internationale, recently published, M. Loewy has recorded certain series of measurements on a photographic plate which have been designed and carried out in a definitely experimental spirit. On this material, which is beyond all

question of a most interesting and instructive kind, he has based an elaborate discussion. His conclusions therefrom lead him to formulate a very clearly defined programme of work in connexion with the *Eros* parallax plates. This programme of measurement is a particularly arduous one, and yet M. Loewy's position of deserved authority makes it appear likely that the attempt will be made at not a few observatories to carry out the scheme in its entirety. It is greatly to be feared that this course may entail a considerable delay in the publication of results. Any such effect would be most regrettable and is most strongly deprecated by M. Loewy himself in his introduction to the circular quoted. No excuse is needed therefore for an examination of M. Loewy's arguments and conclusions with a view of justifying if possible a considerable reduction of the proposed scheme of work. If it could be shown that M. Loewy has underestimated the accuracy of photographic measures, and that for an assigned degree of precision a smaller number of measures would suffice, the result would evidently be, to put the matter on the lowest ground, of considerable economic importance. Considerations of this kind have led me to examine M. Loewy's paper in some detail.

2. Of the three parts into which M. Loewy has divided his paper it is with the first only that I propose to deal. In the second he considers the accuracy of measurements which have been effected simply in the réseau lines imprinted on the plate, and in the third generalises his previous conclusions so as to obtain guidance and lay down definite rules for any circumstances that may arise. But at present it will be unnecessary to go beyond the first section.* The material for discussion has been obtained from a single plate bearing a triple image of each object. The number of stars employed is 82, the magnitude varying between 7·0 and 12·5. Each number recorded is a measure of a y-coordinate, and each has been obtained by setting the micrometer thread on the first réseau line, on each of the three images, and on the second réseau line, and then repeating the five settings in the reversed order. Thus each is the result of four settings on a réseau line and six bisections of a star image. Two tables have been formed, each containing eight measures in this sense of all the 82 stars. In Table I the measures were made by an observer A in successive orientations of the plate 0°, 90°, 180°, and 270°, and were similarly repeated by an observer B. In Table II, on the other hand, the measures were made by one of the observers alone and repeated eight times without altering the orientation of the plate.

* I may, however, take this opportunity to point out a slight slip which occurs on p. 24 of the paper. The mean value of $(2f^2-2f+1)$ when f varies from 0 to 1 is not $\frac{1}{2}$, as given, but $\frac{2}{3}$. The fraction $\frac{1}{2}$ is in fact the minimum instead of the mean value. Numerically the correction may be insignificant, but it is fatal to M. Loewy's contention that the combination of measures which is adopted in order to eliminate the effect of distortion of the film has the further advantage of minimising the consequences of accidental error.

3. By subtracting the mean from each group of eight determinations of y M. Loewy has formed the residuals and determined the probable errors. The values he gives are:

For Table I	$0''\cdot096$
For Table II	$0''\cdot0385$

The disparity is very striking, and its magnitude would hardly have been expected had not M. Loewy's work put the fact so clearly in evidence. That the repetition of measures under exactly similar circumstances would tend to reduce the discordances without increasing the accuracy in proportion is evident on general principles. That the effect of correlation is so noteworthy in this case is, I think, surprising.

4. M. Loewy at once bases on the constants given above a general numerical theory of the errors made in measuring a plate. He distinguishes between two kinds of error, the accidental and what he calls the systematic. The term "systematic" in this connexion is rather unfortunate and perhaps misleading. What is meant is that the irregular outline of an image will create a different impression on the mind of an observer when seen at different times and under different circumstances; on the other hand, as M. Loewy very rightly says, "l'astronome, ayant défini *à priori* ce qui lui paraît être les centres des deux images à comparer, s'efforcera toujours de les pointer aussi bien que possible en restant fidèle à son interprétation primitive, et commettra une erreur constante qui ne sera pas éliminée par la répétition des pointés." Thus we may admit the existence of a source of error which may be to some extent mitigated by repetition of observations under carefully varied circumstances. This is not altogether what is generally understood by systematic error, by which is more usually meant that class of error which is not subject to the axioms of the Theory of Errors, and cannot, therefore, be eliminated by mere repetition of observations. No one could think of accusing M. Loewy of heresy in an elementary matter of this kind, and yet it is rather difficult to follow him when he says (of the systematic error in his own sense): "Cette dernière quantité peut encore renfermer, par exemple, les petites variations que peut subir la couche de gélatine elle-même sous l'action des divers agents chimiques." Anomalous effects arising in the process of developing the plate are clearly systematic in the ordinary and not in M. Loewy's sense. Perhaps the sentence quoted is not intended to bear the meaning I have given it, but it is certainly equivocal.

5. We may follow M. Loewy in denoting the accidental and systematic errors by E_a and E_s respectively, and the total probable error by E_t, and admit that $E_t^2 = E_a^2 + E_s^2$. The number of settings on each image in a given position of the

plate is denoted by n and the number of orientations by n'. Then M. Loewy writes down the general formula

$$E_t^2 = E_a^2/n + E_s^2/n'$$

without further explanation. Surely the appropriateness of this formula is not obvious. If for one orientation we have $E_t^2 = E_a^2/n + E_s^2$ it might be expected that when the operation is performed in n' orientations we should have $E_t^2 = E_a^2/nn' + E_s^2/n'$. But in any case the point happens to be unimportant. It will be convenient to denote M. Loewy's formula for E_t by $E(n, n')$. Now the results derived from the two tables of measures give at once

$$E(2, 1) = 0''\cdot096 \text{ and } E_a = 0''\cdot0385 \times \sqrt{2}$$

It follows at once that $E_s = 0''\cdot088$. M. Loewy has then constructed on this basis a table of probable errors for all values of n and of n' from 1 to 10. From this it is an easy matter to select the most economical way of arriving at any required degree of precision. M. Loewy decides in favour of two settings in each of four orientations of the plate and expects a probable error $E(2, 4) = 0''\cdot058$. The labour involved in carrying out this programme will be four times that expended in measuring catalogue plates for the Carte Photographique, and yet if M. Loewy is right the accuracy will not be doubled.

6. Now there are two objections to the validity of M. Loewy's conclusions which suggest themselves quite naturally. They do not as a matter of fact prove substantial, and yet M. Loewy might have disposed of them by anticipation. He has made no reference to the systematic error of personality; and while he has contrasted results obtained jointly by two observers with results obtained by one alone, he has given no justification for supposing that the two are equally skilful and therefore directly comparable. I have been led in consequence to examine these points, but with a negative result. Addition of the residuals according to each observer for each orientation only served to indicate the following constant errors:

	0°	90°	180°	270°
Obs. A	+0'·0001	+·0008	−·0003	−·0005
B	+ ·0009	−·0001	−·0017	+·0008

These are too small to be of serious account, but they have been removed, as is clearly allowable, before the following errors of mean square were determined:

	0°	90°	180°	270°
Obs. A	0'·0024	·0020	·0024	·0016
B	·0024	·0027	·0022	·0026

These are as well accordant as could have been expected; they lead to a mean probable error 0''·094, which is quite close to

M. Loewy's value. The divergence between the mean probable errors for A and B (0″·08 and 0″·10 respectively) is, however, sufficient to warn us against laying too much stress either on small differences or accidental coincidences between numerical estimates of probable error.

7. There is, however, a striking anomaly which manifests itself in the residuals of B in orientation 90° and 270°, and which deserves mention. If they are summed for the first 52 and last 30 stars separately this is the result :—

	90°	270°
First 52 stars	−0′·0558	+0′·0906
Last 30 stars	+0·0491	−0·0294

Or in the mean

−0″·07	+0″·11
+0·10	−0·05

This suggests that B's measures in 90° and 270° may have by some means become interchanged in the latter half of the list. It is, of course, impossible to decide at what point this may have taken place. But interchanging the later sums as given we obtain for B's personal equation in these orientations the numbers

−0″·06 +0″·10

And if we now make allowance for these constant errors we shall reduce the errors of mean square to

0′·0024 0′·0022

and so improve the agreement with A. But the total probable error will not be materially altered. The explanation I have suggested may perhaps appear rather fanciful. Yet it seems the only alternative to a conclusion so grave that we should naturally hesitate to accept it. This is that a skilled observer in the course of a series of measures is capable, owing to a sudden breach of continuity in his personality, of introducing and maintaining a constant discordance amounting in this instance to nearly 0″·2.

8. My next step was to compare directly the two means of the measures recorded in Tables I and II of M. Loewy's paper. The result of the comparison is set out in column I of the table given on p. 624. The differences are given in the sense Table II–Table I. There is an indication of a constant error in Table II of about +0′·0009, or the same as that of B in orientation 0°. The identity of the observer is not revealed, and it would be interesting, though not very important, to know whether the coincidence is accidental or not. In order to appreciate the

significance of the table of differences it is necessary to realise that each mean is based on no less than forty-eight bisections of star images. I am not sure whether a second observer is to be reckoned as doubling the number of orientations or merely the number of settings. But whether the probable error to be assigned to means from Table I is E(4, 4) or E(2, 8) does not happen to matter. In either case M. Loewy's theory furnishes a probable error of $0''\cdot102$ for the differences between the two means, while a direct determination from the squares of the discordances gives $0''\cdot114$. That is to say, M. Loewy's numbers actually underrate if anything the untrustworthy nature of measures effected in a single orientation of the plate.

9. M. Loewy has then exposed a serious source of fallibility in measures of a photographic plate, and we come now more particularly to the consideration of the remedy he has proposed. This consists in altering the position of the plate between successive series of measures. A good idea of the effect of this procedure will be gained by a comparison of the mean of the measures of A and B in orientations 0° and 90° with those effected in orientations 180° and 270°. The discordances are given in column II of the table below. Counting the number of observers as virtually multiplying the number of orientations we derive from M. Loewy's formula the probable error $0''\cdot082$ for a difference. On the other hypothesis it is $0''\cdot096$ and as actually determined it is $0''\cdot093$. We notice at once that column II has a more satisfactory aspect than column I, especially when we consider that the means employed are based on twenty-four bisections, while column I rests on twice that amount of material. So that up to a certain point M. Loewy's theory stands confirmed, and we must admit that a source of error exists which can be mitigated only by increasing the number of orientations. It certainly seemed to me at this stage of my examination that M. Loewy's views were substantially correct, and yet one might hesitate before accepting his practical conclusion that it is necessary to make measures in four orientations. Exact experiments are even yet wanting which would enable us to draw a line of certainty between the exaggerated fears of some and the excessive confidence of others in regard to the accuracy of the photographic method. But systematic error must of necessity and undoubtedly does exist, though we cannot exactly gauge its magnitude. It might then be justly argued that just as M. Loewy has proved that it is unprofitable beyond a certain point to multiply the number of settings in one orientation on account of the tendency of an observer to repeat his own errors, so a limit may be quickly reached to the number of orientations of value proportionate to the labour expended on account of the existence of the systematic error. And, further, just as the remedy for the one danger is to increase the number of orientations, so the remedy for the second difficulty is to employ a large number of plates. The argument may be

rendered numerically definite by a reference to M. Loewy's own table of probable errors. There we find E(2, 4)=0''·058 and E(2, 2)=0''·073. These are in the ratio of 4 : 5. But when the systematic error is taken into account, even though it be comparatively small, the ratio of the complete probable errors will be much nearer unity. It becomes then a serious question whether it is worth while to double the work of measurement for the sake of so small an advantage. There is one other point which may be alluded to here. The uncertainty which we now see attaches to the mere measurement of a photographic plate will doubtless be regarded by those who are doubtful of the accuracy of the photographic method as strong confirmation of the justness of their scepticism. It may be so. But the advocates of the comparative advantages of direct micrometrical methods may perhaps notice that the argument is rather a double-edged weapon. The magnitude of the uncertainty has manifested itself because it is easy to vary the conditions of observing. But we are just as sharply reminded on the other hand that the mere agreement of observations is a very deceptive guide as to their excellence. It may be something of a scientific platitude to remark that small probable errors may indicate repetition and not avoidance of error, but it is seldom so emphatically enforced as by the material which M. Loewy has put on record.

Table of the Number of Errors between given limits.

Limits.		I	II Numbers.	III
Extreme errors ...	...	−0''·37	−0''·46	None
−0''·24	−0''·18	4	2	3
−0·18	−0·12	9	3	8
−0·12	−0·06	7	7	17
−0·06	0·00	14	14	21
0·00	+0·06	10	16	19
+0·06	+0·12	10	15	10
+0·12	+0·18	8	13	4
+0·18	+0·24	6	4	0
+0·24	+0·30	6	2	0
+0·30	+0·36	4	3	0
Extreme errors ...	...	+0''·41	+0''·37	None
		+0·46	+0·41	
		+0·52		
Probable errors		±0''·114	±0''·093	±0''·057

10. I come now to a new fact disclosed by the examination of M. Loewy's data, and one which I regard as of great interest

and no little importance. It throws a new light on the whole question and it demonstrates the insufficiency of M. Loewy's formula as a complete representation of the facts. The means were formed of A and B's measures in orientations 0° and 180°, and again in orientations 90° and 270°. The discordances are analysed in column III of the table given above. For the purpose of applying the formula of M. Loewy they are based on just the same material as those appearing in column II, and the resulting probable errors ought to be equal or nearly so. On the contrary they are, as a matter of fact, *absolutely different* ! The vast improvement effected by the new grouping of measures is shown much more clearly by the analysis of the discordances than by the mere probable errors, though these latter are of themselves significant enough. Thus our new probable error is exactly half of that given in column I and less than two-thirds of that given in column II. It seems quite possible that M. Loewy's formula may in general apply to results which have been obtained by merely multiplying the orientations at random. But we must now conclude that *by combining measures in orientations* 180° *apart* (i.e. *with the plate reversed*) *we shall effect a much greater improvement of accuracy than by a random choice of orientations.* Before considering the inferences which can be drawn from this principle I set down here some comparison numbers for the three cases :—

	I	II	III
The numerically largest error ...	$0''.52$	$0''.46$	$0''.23$
No. of errors $> 0''.3$	8	6	0
„ „ > 0.2	21	11	3
„ „ < 0.1	35	47	61

This is merely of course a brief epitome of the previous table, but I hope it will assist to place in the strongest possible light the salient features of contrast.

11. Now numbers like these prove with ample clearness that our last mode of grouping the measures must have eliminated some source of error which cannot be removed by combining observations in two orientations at right angles. Hence we can infer something as to the nature of the "systematic error" of M. Loewy, or at least of a considerable part thereof. It must to a great extent *remain constant for each image when the plate is reversed,* for it is eliminated in great part by taking the mean. On the other hand it *cannot be constant either in magnitude or sign from star to star,* since it escaped the search for a truly systematic error. An error possessing these qualities must always be somewhat difficult to detect. Yet serious consequences may follow if we ignore its existence, for in general it will not obey the axioms on which the ordinary theory of errors is founded. In such a case, as indeed in the one under consideration, special precautions are required if we are to avoid its

disturbing effects. I do not mean to say that the whole error, apart from what is purely accidental, can be removed by simply reversing the plate. But so great an improvement can be effected in this way that this method of procedure must be considered a matter of necessity when measures of the highest precision are required. And already the principle has been adopted in practice, although its function has not, I believe, been thoroughly understood. The causes and amounts of the variations of the error in question cannot at present be precisely determined, though they seem clearly to depend in a general way on the character of the photographic image. Some further remarks on this point will be found in § 14 of this note. One suggestion of a practical kind may be put forward here. We may reasonably conjecture that the magnitude of the deviation will be affected by the psychological condition of the observer at the moment. This factor is obviously liable to change; we have already noticed an extremely serious example of the phenomenon in § 7 unless it can be explained away as I have suggested. At any rate it will be of great advantage to curtail as much as possible the interval elapsing between the measures effected in the direct and reverse position of the plate. This consideration will lead us to suppose that a reversing eyepiece may prove an important adjunct to the measuring machine. It is interesting to notice that this feature has been adopted by Mr. Hinks in designing the new Cambridge machine, and that in his description of the instrument (*Monthly Notices*, lxi. p. 448) he says: "A reversing prism in the microscope, by means of which the field of view can be rotated, is very useful in avoiding personality in measurement." Possibly it would not be speaking too strongly to say that some device for achieving the object in view is not merely useful, but indispensable if the highest accuracy is to be obtained.

12. We have now established, as I think, a firm principle on which to proceed. The line of further search is now clear. Evidently we must regard as the unit of measurement, *not* a measure made in a single orientation, *but* the mean of two effected on the plate in the direct and reversed position. If measures are to be multiplied beyond this extent it is not to be supposed that nothing will be gained by changing the orientation. But that question now appears to be rather of subsidiary importance. I have formed the mean residuals according to the basis suggested, and find for the probable values of the divergences from the complete mean:

	0° and 180°	90° and 270°
A	0″·056	0″·049
B	0·061	0·055

from which I deduce the absolute probable errors

A	0″·065	0″·053
B	0·073	0·062

giving 0″·063 for the mean. The errors of B, considered in this aspect, are rather worse than A's; but not to insist too strongly on small differences of numbers in a matter of this kind we may estimate the probable error of an observer who makes double measures in both the direct and reversed position of a plate at about 0″·06. I believe that the accuracy of measurement at which M. Loewy aims can be substantially obtained with the expenditure of half the labour that he proposes to devote to this object; and if his programme is carried out the probable error arising from the process of measurement alone may be reduced to 0″·04, while the result of this increase of accuracy in this one department of the work will not be commensurate by any means with the labour expended.

13. Certain subsidiary comparisons seem to show that the estimates formed of the probable errors of the principal means for A and B are too large rather than too small when combined according to the theory of errors. Let these means be denoted by α_1, α_2, β_1, and β_2. Now we have already found (in column III of the above table) that the probable error of $\frac{1}{2}(\alpha_1+\beta_1-\alpha_2-\beta_2)$ is 0″·057, whereas the numbers of the last paragraph give 0″·063. So, again, in order to determine whether there would be any advantage to be obtained by combining the means for A in one orientation with those for B in the other the probable error of $\frac{1}{2}(\alpha_1+\beta_2-\alpha_2-\beta_1)$ was determined and found to be 0″·062. The probable value of $(\alpha_1-\alpha_2)$ appears to be 0″·077, while calculation from our constants would give 0″·084. Finally the probable value of $(\beta_1-\beta_2)$ is 0″·096, and agrees perfectly with the calculated value. In these last differences, it may be mentioned, thirty are positive and fifty two negative; a fact suggestive of abnormality. As we are here dealing with the principal means we are, of course, independent of the truth or otherwise of the suggestion made in § 7. On the whole, then, we have some evidence that the residual "systematic" error (in M. Loewy's sense) is certainly small after the treatment of the measures which we have been led to adopt.

14. Owing to the peculiar nature of the error mainly under discussion it would be interesting to inquire into its relation to star magnitude. The material contained in M. Loewy's paper is not ample enough to decide this question. But I have rearranged the residuals according to magnitude and derived the following results from the means. The stars here have been placed in three groups, and the number and magnitude given for each.

		31 stars. >12	41 stars. $<12, >10$	10 stars. $<10, >7$
	°	″	″	″
A	0	−0·05	+0·02	+0·10
	180	+0·03	−0·01	−0·19
	90	+0·01	+0·04	+0·17
	270	−0·02	−0·02	−0·08

		31 stars. > 12	41 stars. < 12, > 10	10 stars. < 10, > 7
B	0°	$+0''.04$	$+0''.05$	$+0''.16$
	180	-0.05	-0.11	-0.17
	90	-0.07	$+0.03$	$+0.01$
	270	$+0.11$	$+0.01$	0.00

The material is scanty, and more reliable evidence is to be desired; and yet a glance will reveal a very distinct tendency towards a progressive change of about $0''.04$ per magnitude. The quantity is small certainly, and doubtless negligible in Astrographic Chart work; but in our discussion it is of the same order as the small quantities with which we have been dealing in forming the probable errors. The matter seems quite worthy of further study. At the same time when ampler data are available it would be interesting also to examine the relation of precision to star magnitude. Under the present circumstances it has seemed to me unprofitable to attempt a solution of this question.

15. *Summary of conclusions:*

(1) M. Loewy's contention with regard to the fallibility of measures made in a single orientation of the plate is substantially correct. The facts can *not* be explained by a constant personality or by the inferiority of one of the observers.

(2) A certain improvement in the accuracy of the result can be reached by measuring in different orientations (such as 0° and 90°).

(3) If M. Loewy's estimate of the precision of measures so made be accepted, it does not follow that it is worth while to repeat the measures as many times as he thinks necessary.

(4) By combining measures made in the direct and reversed position of the plate a much better result is obtained to which M. Loewy's formula does *not* apply.

(5) Sufficient accuracy will be obtained by making double measures in these two positions of the plate, and *the labour demanded by M. Loewy may be reduced by one-half.*

(6) The error eliminated by reversing the plate is definite for each image, but may vary both in magnitude and in sign from one star to another.

(7) Some means of reversing quickly is desirable, in case this definite error is liable to change with lapse of time.

(8) The probable error of the mean of double measures made in orientations differing by 180° is about $0''.06$. The residual error (other than the accidental) is small, and up to a certain point measures in this sense will obey the ordinary laws of accidental error.

(9) There is some evidence that personality varies considerably according to star magnitude.

University Observatory, Oxford:
1901 *September* 5.

The Green Flash at Sunset.
By Professor William H. Pickering.

(*Communicated by the Secretaries.*)

As an independent confirmation of the observations described in Mr. Franklin-Adams's paper published in the *Monthly Notices* for May 1901 I would say that in the early part of last June I was watching the Sun set from the deck of a steamer not far from the coast of Cuba. At the instant that the Sun disappeared its last ray suddenly turned from red to a distinct blue. The effect was very striking, and I turned to the first officer of the steamer, who was standing beside me, and asked him if he noticed it. He replied that he did, and that he had never observed it before. The sea was very smooth at the time, and I did not notice the appearance resembling Baily's beads.

Mandeville, Jamaica, W.I.
1901 *July* 3.

Note on Two Stars in the Revised Madras Star Catalogue for 1835.
By A. M. W. Downing, D.Sc., F.R.S.

Dr. Herman S. Davis, who is engaged on the re-reduction of Piazzi's observations, has called my attention to certain errors in Piazzi's Catalogue affecting two stars in the Madras Catalogue. I am much obliged to Dr. Davis for his kindness in communicating these particulars to me.

No. 5835. This is really Piazzi xii. 169. Piazzi has the wrong sign for the declination in his catalogue.

No. 8791. Piazzi's declination is in error by $3' 40''$ on account of his adopting as the place of one star the mean of the observations of the two stars A. G. C. Leipzig II. Nos. 9075 and 9081, with the R.A. of the former of which Piazzi's R.A. agrees.

Taylor obtained one observation of the declination of Leipzig II. No. 9075, at the date 1836·56, with concluded result $+7° 51' 31''·49$. The corresponding R.A. is $19^h 3^m 0^s·23$, with precession in R.A. $+2^s·894$.

These results should be substituted for those in the revised catalogue.

Taylor was misled by Piazzi's erroneous declination, and apparently observed in declination a faint star not far from the erroneous place.
